tainly indisposed to throw away any more time on this discussion; but I may remind those who are interested in it that the question really lies between Prof. Newton and the Royal Commissioners. On the one hand, Prof. Newton, with no practical acquaintance with the subject, knowing nothing of the witnesses or of the circumstances under which they gave their evidence, but taking the Index to the evidence as his sole guide, so completely satisfied himself that the Commissioners had arrived at an erroneous conclusion in believing our sea-fisheries were flourishing that he brought their delinquencies before the British Association. On the other hand, Prof. Huxley (who somehow has obtained the character of thoroughly working out any subject he takes up, so far as he has the means of doing so) and the other Commissioners themselves visited the several fishery stations, ascertained beforehand the nature of the disputes and complaints among the fishermen, and examined and cross-examined the various witnesses. They spent many days separately and together in considering the evidence on the several subjects of their report, and unanimously agreed to this among other conclusions:—

"The total supply of fish obtained upon the coasts of the United Kingdom has not diminished of late years, but has increased; and it admits of further augmentation to an extent the limits of which are not indicated by any evidence we have been able to obtain."

E. W. H. HOLDSWORTH

Athenæum Club, November 25

Examinations in Science

MAY I beg you will introduce to the notice of your readers a grievance urgently in need of a remedy? The grievance is considerable; the remedy simple; and if scientific men will not make it their concern, nobody else will.

A short time ago, when the competitive examination controversy waxed warm, I ventured to enter somewhat fully into the present haphazard system of awarding marks to candidates who competed for posts in the public service. My object then was twofold:—(*a*) to bring facts and figures to bear against the erroneous statements of a few theorists who unfortunately were able to command a great deal of public attention; (*b*) to get fair play for all examinees. My object now, however, is to warn those who advocate the advancement of scientific instruction, that the present faulty method of conducting public examinations (in some quarters at least) tends far more to the depression than to the encouragement of scientific study. Destitute myself of scientific knowledge, and bound to no particular curriculum of instruction, I am obviously not writing from the point of view of a partisan; and if I have joined in the lament of scientific men that insufficient consideration is given in most schools to the teaching of science, it is simply because there are good grounds for the conviction that the higher education of this country is too one-sided.

The point I wish to raise is not whether the grammar and philosophy of science contribute to the training and stimulating of the youthful mind in a greater or less proportion than the grammar and ornaments of the Latin and Greek languages; nor whether so-called technical instruction is being properly administered or injudiciously shelved; but I am asking whether scientific teaching, so however little it be, is adequately encouraged by scientific men in the persons of their public examiners?

Judging from the issues of certain examinations, the candidates for which are drawn from the leading schools, I am satisfied that it is not.

By dint only of considerable pressure are candidates induced nowadays to carry on their school course in science for an additional year or two, so general is the conviction among them that they are merely gambling for marks and that the object of much honest labour will not be attained. In fact, for the particular purposes they have in view, they run the double risk of wasting their time and burning their fingers.

It may, of course, be urged that the ends of science are not furthered by youths who aspire to touch only a modest limit in view of qualifying for public employment; but surely as much may be said of almost every other branch of study. And if this is really the opinion entertained by science examiners, it would be better at once to expunge all scientific subjects from the Government programmes.

But candidates and teachers are concerned only with the rules and regulations that are actually current, and that wiser men have made; and their grievance is that there *is a greater element of uncertainty in the awards issued for science than for any other subject.* Instead of estimating the various science subjects as fractional parts of a wide and comprehensive programme, and of dispensing marks on a fixed and definite plan whereby a given quota of proficiency shall be made to carry the same *relative* weight as a given quota of proficiency in other branches, it would seem that examiners, who, by the way, are constantly being changed, regard their own branch as a distinct entity—set up their own standard of excellence for the nonce—and distribute basket after basket of ducks' eggs among all who fail to reach a very advanced qualifying minimum, forgetting perhaps that meanwhile the classical candidate is receiving his modest or substantial reward according to the character of his work. The position indeed would be pretty much the same as if a classical examiner should announce that no candidate would be entitled to a single mark who did not write a faultless copy of Greek verses! I am prepared to show that this ideal standard has varied to the extent of 50 per cent. in two successive years; nay, more, that science candidates have suffered a loss of 50 per cent. in their marks after an additional year's reading under the best teaching that money can purchase. A case occurred some months ago of a youth who, having won the Huxley and Balfour prizes in Edinburgh, entered his name for an open competitive examination in London. He obtained 64 out of 1,000 marks in his two branches of science, at a moment when from 500 to 600 marks out of a total of 1,500 were being showered upon the classical men. At this particular ordeal "Chemistry" chanced to receive decent recognition, but as this youth's tastes happened to run in another direction he was ignominiously defeated.

Any number of such cases may be enumerated, but perhaps I have said enough to prove that a real grievance does exist.

The remedy is obvious: either to induce the authorities to strike out the words "Natural Science" from their list of subjects, or to arrange for the formation of a committee of science examiners who will devise some plan for fixing, as nearly as possible, a uniform standard, and for distributing marks on equitable principles, after consultation with the classical, mathematical, and other examiners. In default of this I do not hesitate to say that examinees will continue to be trifled with at the most important crisis of their lives; for at these public examinations it is no longer a question whether they gain a scholarship or improve their position at school—it is a question of their future career.

I have yet to state the main point. Setting aside the fact of hardship and injustice, it may be asked how far the present independent and very summary system of dealing with batches of schoolboys can possibly cripple the cause of the technicists who are anxious to press forward the teaching of science. My reply is that science candidates, heartsick with disappointment, will fall out of the ranks and will induce others not to enter them; the belief will rapidly gain ground in the schools that science is "a mistake;" and there is abundant reason for supposing that many a schoolmaster will be only too willing to endorse this opinion. I contend that our public examiners wield the thong that lashes the schools into action, and that we are only just beginning to get fair play for what are called "modern" subjects, but that unless our science examiners apportion their marks in a more just and consistent manner they will simply drive all science candidates "bag and baggage" out of the field. In other words, they will virtually be paying a premium to the schoolmasters for neglecting to carry out the very objects they are clamouring for.

W. BAPTISTE SCOONES

Garrick Chambers, Garrick Street,
December 12

The Rocks of Charnwood Forest

MAY I be allowed a short space in reply to Prof. Hull's courteous reference to my letter on the Charnwood rocks, for I fear that I have failed to make two points in that sufficiently clear? One was, that as the Borrowdale series of the Lake District and the (Lower) Cambrian series of North Wales are *equally* azoic, no correspondence in time with the latter could be inferred for the azoic Charnwood rocks. The argument from absence of fossils surely tells as much one way as the other; indeed, having regard to the similar petrological conditions of the Borrowdale and Charnwood rocks, I think it is slightly in favour of their correspondence. The other point was, that as Prof. Sedgwick's term Cambrian included the Cambrian and Lower Silurian of the survey, his authority could not be quoted in favour of the (Lower) Cambrian age of the Charnwood rocks any more than of their correspondence with the Borrowdale series, unless it could be

shown (I am not aware that it can) that he had definitely correlated them with (Lower) Cambrian beds. T. G. BONNEY

St. John's College, Cambridge, December 11

Self-Fertilisation in Flowers

DR. MÜLLER (NATURE, vol. xiv. p. 571) and Prof. Asa Gray (vol. xv. p. 24) reflect on your abstract of my verbal remarks (vol. xiv. p. 475) on Browallia in a way not particularly complimentary to me. Prof. Gray admits having read the full report, and yet fails to notice that "February 8," is there given as the date of my remarks. Had he not overlooked this, he would not have wondered that I did not see "Hymenoptera and Lepidoptera of various sorts" visiting them. As reported in the *Proceedings* of the Society, I exhibited fresh specimens in fruit at the meeting of that date, which is about mid-winter with us, when these insects are at rest. The plants were of course grown under glass, and when I say "Browallia is not visited by insects, yet seeds abundantly," I am referring naturally to the experience I am describing. If one be justified in taking an unguarded expression, or even a whole sentence, without any regard to the subject matter of its connection, we might have as many "theories" in science as there are sects in religion, all founded on isolated "texts" in Scripture. It is remarkable that in a paper in which Prof. Gray is commenting on hasty observations, in another he should have overlooked a fact like this. I do not say Browallia is never visited by insects, but I do say that they do not visit them *under such circumstances as I was describing*.

Of the *fact* there is no doubt, of the *interpretation* there may be many opinions; and no one respects an opinion by Prof. Gray, when he carefully considers it, more highly than I. Yet I would respectfully submit, that even though an insect were as careful to avoid the "brush" which almost closes the throat, though it were able to be as careful in finding the chink as Dr. Gray was in his manipulations with the hog's bristle, the obstruction of the mouth in the way it is cannot surely be claimed as an arrangement in favour of cross-fertilisation.

Dr. Müller seems to believe that I do not know that "many flowers have recourse to self-fertilisation when not visited by insects." If he will examine the *Proceedings* of the Am. Association for 1875, p. 247, he will find that I have given him the credit of the observation, and the fact itself such consideration I thought it in justice entitled to. The impression which Dr. Müller's expression warrants, that he has not had the opportunity of reading the numerous observations I have placed on record during the last few years, in relation to this and kindred topics, fully excuses him in my mind for his sharp comments.

THOMAS MEEHAM

Germantown, Philadelphia, Nov. 21

On Supersaturated Solutions

IN a paper communicated to the Royal Society last May I described some experiments to show that the open air and the air of ordinary rooms do not generally contain crystals of the various salts which form supersaturated solutions. It has been remarked to me that I did not give the strength of the solutions, so that doubt might arise as to whether the results would hold good for very strong solutions:—The following experiments set that question at rest. I made a very strong solution of sodium sulphate which threw down abundance of anhydrous salt on boiling. When cold a good half inch of anhydrous salt remained at the bottom of the test-tube. Took this into my garden, which is near Bristol. Took up some of the solution in a clean pipette and put drops on the leaves of peonies, which were very dusty, on geraniums, on moss, on the stone coping of Bath oolite, and on the painted woodwork of the railings and garden door. Not a single drop crystallised. Made a drop set quite solid by dropping in earth with the fingers. N.B.—I had been at work with the salt for some time and crystals were probably adhering to my finger. Earth not touched inactive. The drops sank into the moss slowly, remaining quite liquid. Those on the stone were soon absorbed and dried up on the surface; fresh drops put on these remained liquid. Smeared a drop repeatedly with the finger which had been cleansed; inactive, as fresh drops remained liquid on it. Drop on flower-pot, inactive, smeared with finger; when dry inactive to fresh drops. These and other drops on the flower-pot slowly formed a film of 7-atom salt. Stirred the solution with a dry twig picked off the ground, inactive. The drops on the leaves all slowly evaporated, giving the 7-atom salt. Finally, made some of the drops and the original solution crystallise, to prove that they were really supersaturated. These experiments were made both in sun and shade. Weather dry. The test-tube was left open the whole time.

On another occasion I took a flask of sodium sulphate containing a large quantity of the 7-atom salt into the garden in the evening. Put drops on a flower-pot; one only crystallised. Put a lump of dry earth into one drop, and added more solution; did not crystallise. Made a little mud pie by breaking this up with the pipette, inactive; pipette repeatedly inactive in the solution after touching this. Brought a crystal to the earth; crystallised at once all through the mass.

Clifton College J. G. GRENFELL

KARL ERNST VON BAER

SCIENCE has sustained a great loss by the death of Dr. Karl Ernst von Baer, the eminent biologist; he died at Dorpat on November 29, in his eighty-fifth year. Von Baer was born in Esthonia on February 29, 1792, and while yet at the gymnasium became an earnest student of botany. He studied medicine at Dorpat in 1810-14, whence he proceeded to Vienna for the study of clinical medicine, to Würzburg, where he gave special attention to comparative anatomy, and to Berlin, where he studied magnetism, electricity, crystallography, and geology. In 1817 he went to Königsberg as prosector to Prof. Burdach, and two years later he became professor of zoology at the same university. In 1826 he succeeded Burdach in the chair of anatomy, accepted an invitation in 1829 from the St. Petersburg Academy, but returned to Königsberg the following year. A few years later, in 1834 he was again invited to St. Petersburg, where he became one of the most active members not only of the Academy, but also of the Geographical and Economical Societies. Von Baer's writings, marked by philosophic depth, are, on account of their orderly and clear exposition, as attractive as they are generally intelligible. The subject of the origin and development of organic bodies, which had special attractions for him, he did much to clear up. The foundation of his eminence he laid in Königsberg, where he published in 1827 his "Briefe über die Enstehung des Eies," which was soon followed by the important works "Entwickelungsgeschichte der Thiere," and "Geschichte der Entwickelung der Fische." These works, which are yet of great value, have earned for their author the title of Father of Comparative Embryology.

In the summer of 1837 von Baer made a journey of exploration from Archangel to Novaya Zemlya, and his report is still one of the most valuable sources of information upon that island. In 1851 his attention was attracted to the immense Russian fisheries and the irrational methods used. During 1851-6 he investigated the fisheries of Lake Peipus, the Gulf of Finland, and the Caspian Sea, publishing the results of his investigations in a great work in 1859. The name of Baer is connected with more than one improvement in the fisheries, and some important additions were made to the trade, thanks to his efforts. His remarkable work, "Kaspische Studien," has had no rival. It would be impossible to enumerate the various subjects upon which he has thrown clear light in his writings. The laws of excavation of river-beds, the navigability of the Arctic seas, the steppes and forests of Southern Russia, the Glacial period, the Siberian mammoths, the potato disease, were at various times treated by him, and in each department von Baer opened out new and extensive fields of inquiry. His acquirements in zoology, comparative anatomy, embryology, physiology, and anthropology are well known; moreover ethnography, the early history of mankind, archæology, and the science of language will count him among their most eminent students. In his later years, besides various anthropological papers, he published an autobiography (which appeared soon after the fiftieth anniversary—1864—of his scientific career), his "Reden," and "Kleine Aufsätze vermischten Inhalts"